

Value of Meteoric Radiants based on Three Paths.
By W. F. Denning.

With regard to the validity of radiants derived from only three paths, discussed mathematically by Mr. H. W. Chapman (*Monthly Notices*, 1905 January, p. 238), I would like to offer a few remarks from an observer's point of view.

1. I believe that, as a rule, radiants determined from three paths are useless or at least extremely doubtful, especially when the observers responsible for them have not gained considerable experience in meteoric work.

2. There are particular cases, however, where three meteors may indicate good radiants, and these are when the latter are low in altitude or very near the horizon. In such instances the meteors traverse long paths; they have comparatively slow motions ascending, and the discriminating observer can entertain no doubt as to the place of divergence. On the other hand, the greatest uncertainty must attach to short, swift meteors falling almost vertically in low positions. When the flights of these are carried back in the same lines they may each cross a dozen or more active radiants.

3. Under ordinary circumstances it is far from safe to accept centres resting upon less than five paths. If very meagrely supported positions are included in our catalogues and accorded equal weight with others well determined from ten or twenty tracks it will be impossible to get good mean positions, and the records of this branch will soon be burdened with many non-existent showers occasioning doubts and complications very difficult to eliminate.

4. As there are a multitude of showers distributed over the sky some of the radiants lie near together, and there must naturally occur a large number of accidental intersections of three meteors forming false appearances of radiation.

5. Of course much depends upon the experience, judgment, and method of the observer. Those who rely only upon the directions of flight will frequently deduce false positions, as this is an insufficient criterion. Others who carefully sort the meteors according to their individual velocities, streaks, trains, &c., and also consider the length of path and position of the assumed radiant relatively to the horizon, will seldom err.

6. There are very marked differences in the capacity of individual observers. Considerable practice will not make every observer reliable and accurate. Some workers in this department will generally ascertain radiants within 1° or 2° of probable error, while others will ordinarily make mistakes of 5° or 10° , and fictitious radiants will occur so frequently in the results of the latter as practically to negative their value.

7. In estimating the validity of radiants resting on very few tracks it is, therefore, most important to consider the authority

for these positions and the conditions affecting the determination in each case.

8. As a partial safeguard against pseudo-radiants I would suggest that no radiant be accepted depending upon less than five paths *unless the circumstances are special*. It is true that certain meteoric systems—in fact a large majority of them—are so tenuous that to gather five paths from any of these positions may necessitate forty or fifty hours of watching by one observer, and that many such showers must altogether escape detection if the limit of a proved radiant be placed at five members. But I feel sure that it will be best in the end to reject all very slenderly supported showers *indicated under ordinary conditions*. Even the most proficient observers are sometimes led astray by insufficient materials, and meteoric streams are so abundant that it will be safer to recognise such results only as can be fairly well substantiated by adequate data.

9. Exception might occasionally be made in cases where the observer has acquired extensive experience, and where he has accurately registered three or four meteors under circumstances specially favouring the detection of their radiant.

10. Observers should keep a list of feebly suspected showers met with during the progress of observation and endeavour to corroborate them at similar epochs in following years. By combining materials in this manner good and amply supported radiants may often be obtained.

11. I have sometimes secured certain evidence of radiants from only two, three, or four tracks when the positions have been near the horizon. Thus on 1878 July 28 only two paths served to define a centre at 33° — 20° in the eastern region of *Cetus* :—

	G.M.T. h m	Mag.	From	To	Length.
July 28	13 32	1	$2^{\circ} + 0^{\circ}$	$33^{\circ} + 17^{\circ}$	3°
"	14 32	2	$55 + 44$	$126 + 74$	41

In 1886 April–May I recorded three meteors from a radiant in *Scorpio* at about 254° — 20° :—

	G.M.T. h m	Mag.	From	To	Length.
April 30	11 39	4	$249\frac{3}{4} + 34$	$240 + 76$	36
May 5	14 29	2	$287\frac{1}{2} - 5$	$315 + 9\frac{1}{2}$	31
May 6	10 37	2	$233\frac{1}{2} + 26\frac{1}{2}$	$202 + 57$	38

Bishopston, Bristol :
1905 March 18.

On the Spherical Correction of Object-glasses.
By A. E. Conrady.

The performance of object-glasses and mirrors is influenced very considerably by unavoidable residuals of spherical aberration. The usual treatment of these residuals, and of spherical aberration in general, by purely geometrical methods, *i.e.* as causes of a more or less serious confusion of the "rays" forming the image, is decidedly unsatisfactory from the physical point of view, for the undulatory theory defines a perfect lens or mirror as one which brings all light from a luminous point to the focus *in the same phase of vibration*, or which, in other words, transforms the spherical waves sent out by the object into truly spherical waves converging towards the focus. From this point of view any defects in a lens or mirror can become sensible only in so far as they sensibly throw certain portions of the total light *out of phase* with other portions ; and there can be no doubt that the differences of phase with which the different portions of the total light meet at the focus are the only true and absolute measure of the seriousness of any outstanding aberrations.

A considerable amount of knowledge of spherical aberration in this physical sense may be derived from the known peculiarities in the course of the "rays" of geometrical optics when we bear in mind that the latter are properly defined as normals of the wave surface.

Lenses which combine small angular aperture with small dimensions can frequently be so well corrected that either trigonometrical computation or experimental test shows no sensible departure of any ray from the desired direction towards the focus ; and as the sphere is the only surface which has all its normals intersecting in one point, it follows, as a matter of course, that in such cases the physical requirement as to phase-relation is strictly fulfilled.

But in the majority of cases, and always in lens-systems of either wide angular aperture (microscope-objectives) or of large dimensions (telescope-objectives) it is impossible completely to free all zones of the system from spherical aberration in the usual geometrical sense. Such lenses are said to be affected with "spherical zones," and in such cases the phase-relations at the focus remain to be investigated.

In the simplest case, *i.e.* that of object glasses of the type usually found in telescopes, computation shows that when spherical correction is established in the usual way by uniting the paraxial and the marginal rays, all intermediate zones are "undercorrected," or cut the axis nearer the lens. From this and from the above definition of rays we can conclude that the extra-axial parts of waves refracted by such systems are in advance of the ideal spherical waves converging towards the